

Interactive comment on “Study of the heterogeneous reaction of O₃ with CH₃SCH₃ using the wetted-wall flowtube technique” by M. Barcellos da Rosa et al.

Anonymous Referee #2

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This study was focused on the heterogeneous oxidation of DMS by ozone on or in aqueous solutions. This is related to the important role played by DMS in the marine environment as a precursor of sulphuric acid and non sea-salt sulphate. Recently, some studies indicated that the multiphase reaction between ozone and DMS could be unexpectedly high and could therefore potentially compete with gas phase homogeneous process involving OH, NO₃ and even halogenated radicals. This manuscript reports on the experimental determination of the solubility and reactivity of DMS as a function of ionic strength. The latter dependence being only very poorly described. Accordingly, I can only recommend publication once the authors had a chance to comment on the following points and revise their manuscript accordingly.

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- The manuscript should be carefully re-edited as the phrasing is sometime unclear.
- Errors are reported within referring to the uncertainty level at which they are referring (1 or 2 \times 1555, or confidence intervals). Please state the corresponding level clearly at least once within the manuscript.
- Page 1951, lines 15 to 20: Please do not mix chemical equations and text. Choose one option (text or equations) and change your manuscript accordingly in order to increase the clarity of the document. This remark holds throughout the manuscript which should be revised accordingly.
- Page 1953, line 18: Is figure 1 really showing any equilibration between the gas and aqueous phases? When looking at Figure 1, I do not see any "plateau" showing that the liquid film is attaining the Henry's law equilibrium?
- Page 1954, line 6: How was the interpolation made? In figure 1 it is really unclear that the DMS concentration decreased by a simple exponential.
- Page 1954, line 13: In the experiments devoted to the heterogeneous kinetics measurements, were both phases "saturated" with DMS or did some fraction of that compound desorbed from the liquid film? If yes, how can then the kinetics be measured? Additional information may be required here to clarify the experimental procedure.
- Page 1956, line 6: The effect of viscosity (among other) should be mentioned here in order to highlight that the diffusion coefficient may not be constant from one solution to the other.
- Page 1957: the Henry's law coefficients should be summarized within a Table.
- Page 1957: the unities in equation (7) should be stated as the text now mentions ionic strength in molalities but all other concentration are given in molarities. Being clearer here may avoid some confusion.
- Page 1958: why attributing the lower rate constants to experimental problems? Did

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the authors tried to display the rate constant at one temperature as a function of ionic strength? In fact, in NaCl solutions, the activity of dissolved gases may be lowered and therefore chemical reactions may proceed with lower rates. I would strongly encourage here a deeper discussion of the effects of ionic strength.

Minor remarks

- Abstract, line 15: make sure that the appropriate symbol is used for the uptake coefficient i.e., γ ;
- Page 1953, line 2: "ozone free"
- Page 1953, line 7: "by Kontron", is sentence is not clear.
- Page 1953, line 9: What is a "noise limit/analytical"?
- Page 1953, line 11: "and gaseous phases"
- Page 1953, line 16: of gaseous DMS
- Page 1953, line 18: WWFT is not yet defined
- Page 1953, line 22: "(by 140 cm)", to
- Page 1955, line 21: check the spelling within equation (3). Is really the latter equation referring to the number of molecules being absorbed? It is rather a reactive uptake generated by the chemical conversion of the in-coming gas.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1949, 2003.

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